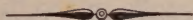


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A DECISIVE FACTOR IN THE ASSESSMENT OF EVIDENCE¹

BY G. N. M. TYRRELL

THE purpose of the following argument is to show that in the selection of evidence in psychical research one very important factor has been largely ignored. The primary aim of the psychical researcher is to collect evidence and to carry out experiment, making both as water-tight as possible. But he begins to do this without previously criticizing the situation in which he proceeds to act. He regards the public as being divided into a credulous section on the one hand and a sceptical section on the other, with a tiny minority of truly balanced individuals in the middle, of which he is of course one. But a little reflection is sufficient to show that the situation is not as simple as that; for the person who is careful and sceptical in psychical research accepts evidence of a similar and hearsay type in everyday occurrences and, strangely enough, this lapse into credulity is justified on the whole. Again, the determination on the part of the investigator to be wholly objective in his methods can lead to the worst kind of subjectivity, not only in psychical research, but in every research of a far-reaching kind: for while the conscious attention is projected into the outside world, the internal working of the mind attempts to force upon the facts an interpretation that is suited to its own structure. The lesson that this teaches is that in any such far-reaching project as psychical research we should keep an eye on the workings of our own minds.

One curious feature, visible in the highly sceptical critic, is that he reveals his attitude far more by his *behaviour* than by what he actually says. This applies not only to the critic of psychical

At the Editor's request, Mr Tyrrell kindly prepared this abbreviated version of the paper which he read at a meeting of the Society on 21 September 1950.

research but to human beings at large. For example, some philosophers who have a certain acquaintance with the *Proceedings* and *Journal* of the S.P.R., when writing about such a thing as the scope of knowledge, assume that the physical sense-organs provide all the information we have and, if they mention extra-sensory perception at all, they skate lightly over it as if it were a matter of incidental triviality and not a fact of central importance, which it must be if it exists at all. Something other than pure reason is obviously at work deflecting their arguments towards a predetermined end. It is *behaviour* rather than overt logic which reveals their true attitude.

If we look back over the past history of psychical research, we cannot help asking ourselves why the subject has made such slow progress in comparison with other subjects—why, after seventy years of careful research, there is still no more than a handful of people at work on it. Why do its funds remain so painfully small when many other projects of less importance in an ultimate sense have made outstanding progress? The answer seems to be that the great majority of people by-pass the subject, impelled by some reluctance or repugnance in themselves, and turn to other things. There may be—in fact there are—cogent arguments which show that the paranormal is of first importance for human knowledge, but these arguments do not make any impression, and it becomes clear that it is *behaviour* and not rational argument which is the decisive factor in the general attitude towards psychical research. In other words, there is an innate instinct which determines peoples' behaviour; so that when the paranormal is broached the general attitude to it is not properly rational. When instinct is in the saddle, we cannot be surprised if the mind tends to be impelled towards foregone conclusions, and can only be weaned from these conclusions slowly and painfully by continuous repetition of the same evidence and the same experimental results.

Another thing which brings to light this sub-rational influence is the light and casual way in which highly intellectual people deal with this subject. Professor A. D. Ritchie, for example, when treating of psychical research, wrote as follows :

... that a number of very queer and obscure phenomena have been observed that do not fit in well with orthodox theories about bodies and minds and their relations. These phenomena can be interpreted in terms of a theory of 'spirits' but they could equally well be interpreted otherwise and with a saving of gratuitous hypotheses. It seems that one must accept either telepathy or clairvoyance as a fact and more probably both as independent facts. Well, granted telepathy and clairvoyance and granted too the possibility of a certain amount of distortion of the temporal sequence of events, so that what is in the

future for one person's experience is not always in the future for another's, it seems possible to account for all alleged 'spirit' communications. It can, perhaps, be done by means of telepathy and clairvoyance without temporal distortion, or by telepathy and temporal distortion without clairvoyance. The point is that the 'spirits' have never reported anything which has not been already known to some living person, or about to be known in the near future or available in written documents, or by means of some already existing material evidence.¹

The interesting thing about this quotation is not the kind of theory that Professor Ritchie supports but the light-hearted casualness with which he deals with telepathy, clairvoyance, precognition, etc. If these functions of the mind are facts, then they are central to our knowledge of the mind and should not be injected into arguments until they have been made the central theme of inquiry. Imagine the criticism that would be aroused if any 'normal' subject were treated in this casual manner or brought into serious argument without careful study. One can almost hear an inward voice saying: 'This is only 'paranormal' stuff: it won't enhance your reputation if you take it seriously!'

From the rational standpoint, what *reason* is there for lightly dismissing the paranormal? There is surely no valid reason; on the contrary, there is every reason for scrutinizing it with the utmost care; for if the evidence is valid, it is of the greatest consequence for science, philosophy, and human knowledge in general: in fact, it throws a vivid light on the entire human situation, and throws it into a perspective which is quite different from that commonly accepted today. One would think that this would act as a spur to every thinking person and make him wish to probe the paranormal to the bottom. But the fact is that while the normal stimulates curiosity the paranormal kills it. It does even more than kill curiosity: it engenders a *resistance* to serious consideration of it. When Dr Rhine, and others in this country, carried out quantitative experiments in extrasensory perception, in which the odds against chance rose to astronomical proportions, the general reaction was to find a way of escape from the conclusion to which the evidence pointed, not an enthusiastic desire to repeat the experiments all over the world and so to install a new piece of knowledge of the highest importance.

If we take a wide survey of the subject, it is surely as plain as a pikestaff that the common reaction towards evidence for the paranormal is not prompted by the purely rational faculty of the mind but is prompted by something *below* the rational level—something

¹ 'Theories of Immortality', *Philosophy*, vol. 17, no. 66 (April 1942) p. 119.

that is very nearly akin to instinct. Many people would be terribly shocked at the suggestion that men of science could have the balance of their minds upset by deep-seated instincts of whose existence they were unaware. It would be objected that if there are instincts of this kind, psychologists would have discovered them ; and also that science could not have achieved its enormous successes if instinct had the power to interfere with reason. The answer to this is highly illuminating if we take tight hold of ourselves and refuse to push it away. Psychologists have failed to realize the presence of this instinct because they are as much under its influence as anyone else. It is not like the individual complexes with which psychiatrists deal : it is not even a group-characteristic belonging to scientists (although it is true that science has reinforced it) : it is a racial characteristic belonging to the whole of mankind—a product of mental evolution. For evolution did not stop short when it had adapted our bodies to their physical surroundings : it adapted our minds to their surroundings as well. But the latter process was far more subtle than the former : it was part of the instinctual suggestions themselves that they should be concealed from consciousness. As for the argument that science could not have succeeded if reason had been influenced by instinct, the answer to that is that science succeeded precisely because instinct did interfere in the processes of the mind. It was because the mind had been pre-adapted to deal with the external world that science was so successful in every practical direction.

A question that inevitably inserts itself at this point is : How, if this instinctive suggestion is universal and invisible, can anyone become aware of it? It is a fundamentally important question ; but for the moment it must be regarded as a side-issue, for there is no space in which to deal with it. Very briefly the answer is that the human mind is not a monad but is more akin to a spectrum. It can attain to different levels of awareness. Intuition and inspiration are examples of this. Only the practical level of the mind is wholly dominated by adaptive instinct ; yet the latter rises into and influences higher levels—that of intellectual thought, for example—and deflects its reasoning towards foregone conclusions.

To go into the nature of the adaptive instincts which enter into the structure of the mind, or even to follow the work of psychologists, such as C. G. Jung, who has taken steps that should have led him in this direction, is impossible here. But there is one feature of this instinct which bears on psychical research, as well as on many other things. This is the instinctive resistance put up by the mind to anything which comes from beyond the province of the senses. Why nature should have instilled this resistance into us

would entail a lengthy explanation ; but the main fact is that the sense-world *had* to be accepted as all-inclusive, otherwise the world would not have appeared to be simple enough for the primitive mind to grow and to act in. It is because of the pre-adaptation of the human mind to the physical world that every question that arises in practical life is capable of being clearly and finally answered.

The effects of mental adaptation can be traced in science, in a great part of philosophy, and in every department of practical life ; but we are here concerned with their influence on psychical research. Throughout the history of the S.P.R. there have always been those who were cautious about evidence and those who went further and tried to explain it away ; and within recent years, the latter tendency has become more pronounced. There are at least three ways in which the tendency to explain away shows itself in the records of this Society ; and these are to be found in external criticism as well. One of these is to inflate the explanatory powers of chance so that chance in relation to the paranormal is held to be capable of accounting for a great deal more than it is in the field of the normal. Even statistical figures do not dispose of this inflationary tendency. The position is quite irrational ; but instinct, when it causes the mind to act irrationally, can at the same time blind the mind to the fact that it is doing so. The same tendency comes out in the criticism of evidence when the weaker features are brought into high prominence and the stronger features thrust into the background. Again, a suggestion is sometimes found running through a criticism of paranormal evidence which is lacking in definiteness yet sullies it through innuendo, so that the reader is left with the feeling that something is wrong somewhere, though it is not clear where. For example, if careful tests were made with a physical medium, which pointed strongly to some of the phenomena being genuine, while a single case with the same medium pointed to fraud, this would be worked into a suggestion that it undermined all that the medium had produced. It is, of course, perfectly right that in psychical research the evidence should be as good as possible ; but there is a point, not at all easy to detect, at which caution lapses into specious argument.

It may be said that it is perfectly rational to demand a higher standard of evidence in psychical research than in any other subject because the paranormal is much more improbable than the normal. This brings us to the heart of the matter. Is the paranormal more improbable than the normal? Many people would say that it obviously is ; for normal things happen all around us and we understand them and know their laws ; but the paranormal only happens occasionally ; and when it does it seems to break the laws of the normal. But there is no cogency in this

argument unless we make the assumption that nothing can act into the world from beyond it. If things did act into it, they would very likely be occasional and might disrupt or modify physical laws. Thus the paranormal is not improbable unless we have strong proof that our senses reveal, in principle, the entire universe. We have no proof of this at all, *but we have a strong, instinctive urge to take it for granted.*

When assessing evidence in psychical research the principle which is surely rational is that the more improbable the alleged facts are the stronger should be the evidence needed to prove them. The critical person, who nevertheless accepts flimsy evidence about things in ordinary life, would probably defend his action by saying that there was nothing improbable in that for which he accepted weak evidence; therefore weak evidence was enough. The principle is that the strength of the evidence should increase in proportion to the improbability of that which it supports.

But when we turn to psychical research, or to any subject which deals with what is held to be improbable, and adopt this principle, we are at once faced with a difficulty. *How* improbable are the alleged facts? We must surely know this before we can decide on the standard of evidence required: and there is the added complication that we cannot fix a standard for the whole of the paranormal. Some paranormal features may be more improbable than others, so we must vary our standards of evidence throughout the subject. Many people would be likely, for instance, to regard telepathy as more probable than communications from the dead; and the phenomena of the seance-room have their own degree of improbability again. But these differences in probability are not the main difficulty. The main difficulty is that people are not agreed as to *how* improbable any of these paranormal features are. This inserts a subjective element into the problem and prevents us from deciding beforehand how strong the evidence need be; for the improbability of anything depends on general agreement about it.

Let us ask this question: How improbable is telepathy? Some people regard telepathy as a matter of course; others regard it as not very unlikely, while others, again, react towards it as if they regarded it as an impossibility, though they probably do not say so in so many words. How then are we to decide *how* improbable telepathy is? If we cannot decide, how can we fix on the standard of evidence required to prove it? Should we agree with the first class? If so, hear-say evidence would be sufficient as it is for countless things in everyday life. Or should we agree with the middle class? In that case the quantitative experiments would long ago have convinced us. Or should we agree with the third class?

n that case, why not say outright that telepathy is impossible and that no amount of evidence can prove it?

This brings us to the heart of the problem. There is no *reason* why the universe should not extend beyond the scope of our senses, and consequently no *reason* why things of an utterly strange and incomprehensible kind, such as telepathy and precognition, should not occur there and should not give rise to sporadic effects within the sense-world. But an ingrained instinct of a racial type has been instilled into us by nature in order to keep our minds focused upon the world of our senses ; and this urges us to reject the possibility of any further extension of the world.

The problem of psychical research is thus not how to obtain evidence which satisfies the rational mind (supposed to be in a state of perfect balance) but how to obtain evidence which will vanquish our racial instinct, which continually attempts to make our reason explain the evidence away. This is why psychical research makes such slow progress, why so few treat it seriously, and why it does not invoke a general enthusiasm for further inquiry.

There are more complications. The power of this racial instinct varies in different individuals because the human mind is graded, and elevation towards the intuitive level weakens the hold of instinct. Also, those who are usually termed credulous placate this instinct by drawing the paranormal into this world and 'normalising' it.

To sum up the present argument : an essential condition for further progress in psychical research is that those engaged in it should recognize the existence of this racial instinct and the subtle part it plays.

DISPERSION OF SCORES IN ESP EXPERIMENTS

BY D. J. WEST

LAST September arrangements were made to test five friends for ESP after they had taken a drug which was supposed to induce a euphoric mood. The experiment did not fulfil its intended purpose, since in no case did the drug produce the desired effect. The results were nevertheless interesting.

The ESP experiment took the form of a clairvoyance test. Packs of ESP cards (five of each symbol) were shuffled by hand and the card order recorded before each session by Miss Elizabeth McMahan of the Parapsychology Laboratory at Duke University who was in London at the time. During the session the target

pack was enclosed in a small wooden box. The subject was asked to call out his guesses 'down through' the pack while the experimenter wrote down the calls. After each run of twenty-five calls the cards were taken out of the box and the calls checked in the subject's presence. One evening session was devoted to each subject. At each session twenty-five runs were carried out excepting at the third session, which was cut short after twenty runs. The results are summarised in Table I.

TABLE I

<i>Subject</i>	<i>Sex</i>	<i>No. of Runs</i>	<i>Deviation</i>	<i>Critical Ratio</i>
J. N.	M	25	+18	1.80
L. D.	M	25	-25	2.50
J. C.	F	20	+13	1.45
Y. A.	F	25	-13	1.30
B. H.	F	25	-14	1.40

$$\Sigma(C.R.)^2 = 15.25 \text{ (5 degrees of freedom). } P = .01$$

Table I shows a significant dispersion of subjects' scores from chance expectation, suggesting that some of them used ESP to score positively while others had a tendency to use their ESP to avoid the target and produce negative scores.

This avoidance of the target, when the declared aim is to score as much as possible, might be described as a disguised response. Other disguised responses have been reported, such as *Displacement* (that is, hitting a target ahead or behind instead of the one aimed at), *Salience* (that is, heaping up of hits at the beginning and end of a run with a deficiency in the middle), and *Consistent Missing* (that is, naming a particular symbol wrongly, such as calling cross whenever circle is the target). These 'effects' may all be disguised responses. It is as if the subjects were like criminals, guilty of revealing this knowledge of the targets, yet unable to prevent this knowledge influencing their calls in devious ways. Of course, this is as yet only speculation.

Alternate positive and negative scoring is a situation which has been encountered in the investigations of Dr Betty Humphrey and Professor Gertrude Schmeidler. By the use of psychological tests predictive of the likely direction of each subject's score, these experimenters have succeeded in subdividing seemingly null data into significantly different positive and negative scoring groups. It occurred to me that if a number of subjects were tested for ESP under constant and favourable conditions, the positive and

negative scoring tendencies of the individual subjects might be sufficiently consistent to produce a significant dispersion of their scores. If the number of trials per subject were too few, as in most of the American work, the effect would not have a chance to display itself. On the other hand, if the subjects were made to do too many runs, the proverbial decline phenomenon might set in and the initial effect be diluted to insignificance by continuous null results. The effect obtained in the small experiment with five friends might have been due to a combination of favourable psychological conditions and choice of a more or less optimum number of trials.

In order that this possibility could be further explored, Mr J. Fraser Nicol and Mr Edward Osborn kindly lent me some ESP data which they collected in 1948-9.¹ Their experiments took the form of card-calling tests with individual subjects under GESP conditions. The investigators were alive to the desirability of providing an encouraging, unstrained atmosphere. The

TABLE II
NICOL-OSBORN DATA

Subject	Sex	No. of Runs Completed	FIRST 16 RUNS		SUBSEQUENT RUNS	
			Deviation	Critical Ratio	Deviation	Critical Ratio
E. F.	F	58	- 4	0.500	+ 11	0.85
A. K.	M	80	- 10	1.250	- 3	0.19
W. J.	M	16	+ 11	1.375	—	—
A. N.	F	102	- 18	2.250	- 45	2.42
M. B.	F	77	+ 10	1.250	+ 11	0.70
D. W.	M	16	- 10	1.250	—	—
E. N.	F	58	+ 20	2.500	- 10	0.77
L. N.	F	30	+ 14	1.750	+ 6	0.80
J. N.	M	26	- 6	0.750	+ 8	1.28
M. T.	M	27	- 7	0.875	- 6	0.90

Total dev. = 0

Total dev. = - 28

$\Sigma(CR)^2 = 22.53$ with 10 degrees of freedom ($P = \text{approx. } .01$).

$\Sigma(CR)^2 = 10.79$, with 8 degrees of freedom (*insignificant*).

¹ The data also included some obtained by Mr Nicol alone. These are the experiments which were referred to in the Society's Annual Reports for 1948 and 1949.

experimental precautions were adequate. In most of the tests agent and subject were in separate rooms communicating by means of a signal light. The target sequence was determined by random numbers. The total result for all subjects showed no significant deviation. (In a total of 15,769 trials the deviation was only 39.2).

Before seeing this data I decided upon sixteen runs as an arbitrary figure likely to be somewhere near the optimum number of runs per subject. Table II shows the results obtained by those of the Nicol-Osborn subjects who completed sixteen or more runs. The table is divided into two sections, the first sixteen runs and subsequent runs.

Table II shows that in spite of the null result in the total, and in spite of the poor performance in later trials, the subjects do display to a significant extent the same dispersion of scores as was seen in Table I. A more accurate method of demonstrating the differing performances of the subjects is the analysis of variance. Each subject's scores are arranged in a column of figures with sixteen entries (one for each run) in chronological order. When the ten subjects' scores are put together a table results with 160 cells. The subsequent course of the analysis is shown in Table III.

TABLE III
FIRST 16 RUNS OF NICOL-OSBORN SUBJECTS

Source of Variance	Degrees of Freedom	Sum of Squares	Mean Square	Variance Ratio, F	P
Between Subjects (Columns)	9	90.1	10.0	2.60	.01 approx.
Within Subjects (Rows)	15	61.2	4.08	1.06	(insignificant)
Residual	135	520.7	3.85		
TOTAL	159	672.0			

The variance ratio in Table III indicates a clearly significant tendency for the scores of the different subjects to vary, some above chance, some below.¹ The table also shows that there was

¹ The scores used in this analysis form a discontinuous variable with a limited theoretical range from 0 to 25. It is therefore appropriate to use in place of the raw scores the converted value θ , where $\theta = \sin^{-1} \sqrt{x/25}$. An analysis of variance was performed on the first 16 runs of each of the 5 subjects in the drug experiment. Using the raw score the variance ratio between subjects was found to be 3.11, $P = .02$. Using the angular transformation $F = 3.21$, $P = .02$. The indication is that angular transformation would make no substantial difference.

no significant variation within subjects, that is no consistent variation common to all subjects with lapse of time. In other words, in the Nicol-Osborn work, as well as in the first experiment with five subjects, some people were using their ESP to score substantially above chance, while others were giving a sort of disguised response, using their ESP to avoid the target and score below chance.

Over the past year a large mass of data from home-testing experiments has been collected by Mr G. W. Fisk. The participants were advised to carry out the tests under GESP conditions and to employ hand-shuffled packs. Since there were no investigators supervising the tests, and the experimental precautions were not rigid, the results, whatever their nature, could never be regarded as more than suggestive. Any subject who achieved an initially promising score was encouraged to continue. There were 177 subjects in all, of whom 134 carried out 8 or more runs and 75 completed 16 or more runs. The results, so far as direct hits were concerned, were null (see p. 369).

TABLE IV

HOME-TESTING EXPERIMENTS. FIRST 16 RUNS. 75 SUBJECTS.
ANALYSIS OF VARIANCE

<i>Source of Variance</i>	<i>D.F.</i>	<i>Mean Square</i>	<i>F</i>	<i>P</i>
Between Subjects	134	4.80	1.19	0.1 <i>approx.</i>
Within Subjects	15	3.03	<i>insignificant</i>	
Residual	1110	4.02		

An analysis of variance was performed on the direct hit scores of the 75 of these subjects who completed 16 runs. Subsequent runs were not included. The result, shown in Table IV, gives no indication of a dispersal of scores as was found in Tables I and II.

The Fisk data were interesting on account of the significant displacement effects. It is important to emphasise, however, that although the total score was statistically significant the scoring average was only very slightly different from chance expectation. The average deviation per run on forward displacement was -0.12 . This tends to confirm the view that one cannot expect a large yield from card-calling experiments using unselected subjects without attempting to introduce strong motivation in each of the participants. In this connection Dr Rhine recently wrote advising a psychologist who was about to embark upon ESP

tests to the following effect: It is important to remember that ESP tests are no mere passive measurement such as you are accustomed to making. It is likely to help most subjects to challenge them to get as many as possible of the symbols correct. Unless the subject is strongly motivated himself, the tests call for the liveliest methods and strongest pressure the experimenter can properly bring to bear.

SUMMARY

Other investigators have demonstrated positive and negative scoring trends in groups of subjects selected by psychological testing. A small ESP experiments with five friends showed a significant dispersion of subjects' scores. This led to the suggestion that, given the right conditions and an optimum number of runs, the positive and negative scoring trends of each subject might reveal themselves as a significant dispersion of scores without any psychological test having been applied. This idea received some confirmation from an examination of the data collected by Nicol and Osborn. No such effect was found in the large body of data from home-testing experiments. The matter would probably repay further investigation by experimenters who have suitable data in their possession.

AN EXPERIMENT IN PRECOGNITION

REPORTED BY G. N. M. TYRRELL

MRS V. M. Austin, a member of the Society, asked me some time ago if I could suggest an interesting experiment in precognition. I replied that it would be a good thing, I thought, to introduce some process of randomisation between a prediction and its fulfilment in order to see whether this would prevent the fulfilment from taking place. The following is an epitome of the experiment. Mrs Austin took a mascot, a small black cat, to Mrs Methven, a medium, and asked her to write down impressions as to what would happen to it in the future.¹ These she sealed up in an envelope without looking at them. This was done on 27 September 1946. One or two nights later, Mrs Austin wrote the names of thirty acquaintances on slips of paper and mixed them up in a hat. She then got a friend to draw one of them, and the name

¹ Mrs Austin tried the experiment on three occasions. The first two, which were not with Mrs Methven, were failures.

on it was found to be Miss Graham (pseudonym). The mascot was at once sent to her, and Mrs Austin opened and read the medium's statements and filed the paper away. Miss Graham sailed for a foreign country on 25 March 1949. She was a civil servant in the employ of a foreign government, and she went to that country partly in order to arrange her retirement from her post. In May 1949 she returned to England. She had intended to travel on the outward journey in a freight steamer alone, but on hearing that her passage would be paid, she went by liner, where she met a friend and travelled with her.

Mrs Austin then sent questions based on Mrs Methven's statements to the other twenty-nine persons whose names had been in the hat in order to find out to what extent these statements applied to them. Mrs Methven's statement is given in full below (A). It is followed by list B, which contains the medium's statement as put to Miss Graham by Mrs Austin, with Miss Graham's annotations shown in italics. List C contains the questions put to the remaining twenty-nine persons.

A. 'The article in question will be carried to a foreign country. Will bring good luck. Two people are travelling together. A great deal of activity. Many new links will be made. I feel will return to this country again perhaps for retirement. I feel you must take care of your health, nothing to be anxious about, chest seems a little difficult at times, able to overcome much with right thought.'

B. '(1) The article in question will be carried to a foreign country. *Wrong.* (2) Two people are travelling together. *Correct.* (3) A great deal of activity. *Yes.* (4) Many new links will be made. *Yes.* (5) Will return to this country again. *Correct.* (6) Perhaps for retirement. *Yes.*'

C. '(1) Did you travel to a foreign country between September 1946 and June 1949? (2) If so, did you travel with a single companion? (3) If so, did you return to England within this period? (4) If so, did your journey bring marked good luck? (5) If so, did you have an exceptionally active time abroad? (6) If so, did you make many new links during this period? (7) If so, was the journey connected with your professional retirement? (8) Even if you did not go abroad, during this period, did you retire or make arrangements for your retirement during this period?'

The chance question was roughly tested by sending list C to the other 29 persons whose names had been in the hat. The gist of the result was that in 45 cases statements of the medium applied,

while in 158 they did not. The whole of the eight statements did not apply to any one of the 29. One of the 29 did, however, make a journey which was connected with retirement. Three out of the 29 had arranged to retire during this period but had made no journey in connection with it.

It has been suggested that the experiment contained the following defects in design : (i) that list B given to Miss Graham to annotate did not correspond exactly with the statement (A) made by Mrs Methven in that it omitted the references to good luck and health ; and (ii) that while Mrs Austin divided list B into six parts, the list (C) given to the twenty-nine persons for annotation was divided into eight parts and omitted the point about health. It has been suggested that as a result the applicability of Mrs Methven's statement to Miss Graham on the one hand and to the twenty-nine persons on the other cannot satisfactorily be compared.

While Miss Graham did, in a later letter, make clear that she had had good luck, it was admittedly a fault that the medium's statement about health was not circulated. This statement did not apply to Miss Graham and was in fact the only mis-statement made by the medium. For the first item in list B, which was there marked wrong, was afterwards found to be right. Miss Graham had inadvertently taken the mascot abroad with her in an out-of-the-way pocket of a bag and only discovered this after her return. As regards the first part of point (ii) : the two additional questions in list C were (4) and (8). The former, which concerned good luck, is dealt with above ; (8) would not have been applicable to Miss Graham as it was known that she had gone abroad.

Perhaps it should be added that Mrs Austin told me in a letter that when Mrs Methven was presented with the cat-mascot, she first made statements about the former environment of Mrs Austin, who had, up to then, been its owner. These statements were correct. Also, during the winter which followed the presentation of the mascot to Mrs Methven, Mrs Austin had a long illness of combined bronchitis and influenza, to which the statement about health might quite well refer.

I think Mrs Austin is to be congratulated on the achievement of this experiment and it is to be hoped that, despite the doubt between chance and the paranormal in this case, more work on precognition along these lines will be done.

HOME-TESTING ESP EXPERIMENTS

A PRELIMINARY REPORT BY G. W. FISK

SINCE quantitative experiments in extrasensory perception started in Great Britain, a mere handful of subjects has been found who have been able to maintain a consistently high rate of scoring. It was in an effort to discover promising subjects that in January 1950 arrangements were made for the carrying out of simple ESP tests by members of the Society in their own homes. An instruction sheet was drawn up, and this, with numbered scoring sheets and packs of ESP cards, was sent to every group of two or more people who were willing to give each other and their friends card-calling tests. It was recommended that Agent and Percipient should be separated by a screen measuring at least three feet square or by its equivalent, that the Experimenter should signal by tapping when the Agent looked at each card, and that 200 calls should be made at each session. It was suggested that any subject scoring ninety-two or more (or sixty-eight or less) in 400 guesses should be regarded as promising, and that further tests with him should be carried out. As the experiments were not supervised, it is, of course, legitimate only to regard the results as suggestive, however significant they may appear statistically.

	<i>Backward Displacement (-1)</i>	<i>Target Hits (0)</i>	<i>Forward Displacement (+1)</i>
Total Hits	13116	14026	13081
Expectation	13416	13975	13416
Deviation	-300	+51	-335
Critical Ratio	2.89	0.48	3.24
P	.004	.32	.001

Up to the end of October 1950, 177 subjects had been tested, and the total number of runs (twenty-five calls to each run) was 2,795. No individual subject was found whose scores were consistently significant either on the target or in either displacement direction. Taking the results as a whole, however, there were significant negative scores on both +1 and -1 displacement. An examination of a substantial sample of the data for +2 and -2 displacement indicated that a negative trend, similar to that discovered in the +1 and -1 scores, was present, though not to a significant degree.

Statistical tests have not yet been applied, but by inspection it appears that the negative deviations on displacement were well distributed throughout the data and were not due to the anomalous performances of a few subjects. It must be emphasised that 'closed' packs were used and that the scores in the direct target and displacement positions are not statistically independent of each other. The close conformity of the direct hits with chance expectation is interesting. In experiments conducted by different volunteers and in a variety of conditions, one might have supposed that spurious deviations would be likely to arise from such normal causes as lack of care in recording, sensory leakage, etc.

The search for promising subjects still goes on. I hope that any readers of this *Journal*—whether members of the Society or not—who have not taken part in these experiments but who wish to do so will get in touch with me (6 Ditton Grange Close, Ditton Hill, Surrey).

THE FRAUDULENT MEDIUMS BILL

ON Friday, 1 December, this Bill was read a second time in the House of Commons. Proposed by Mr Walter Monslow (Labour, Barrow-in-Furness) and seconded by Mr T. J. Brooks (Labour, Normanton), the Bill, in the words of the preamble, is designed to 'Repeal the Witchcraft Act, 1735, and to make, in substitution for certain provisions of section four of the Vagrancy Act, 1824, express provision for the punishment of persons who fraudulently purport to act as spiritualistic mediums or to exercise powers of telepathy, clairvoyance or other similar powers'. It contains the following clauses:

- 1.—(1) Subject to the provisions of this section, any person who—
 - (a) with intent to deceive purports to act as a spiritualistic medium or to exercise any powers of telepathy, clairvoyance or other similar powers, or
 - (b) in purporting to act as a spiritualistic medium or to exercise such powers as aforesaid, uses any fraudulent device,
 shall be guilty of an offence.
- (2) The foregoing subsection shall apply only where a person acts for reward; and for the purposes of this section a person shall be deemed to act for reward if any payment is made in respect of what he does, whether to him or to any other person.
- (3) A person guilty of an offence under this section shall be liable on summary conviction to a fine not exceeding fifty pounds or to imprisonment for a term not exceeding four months, or to both such fine and such imprisonment, or on conviction on indictment to a fine not exceeding five hundred pounds or to imprisonment for a term not exceeding two years or to both such fine and such imprisonment.

(4) No proceedings for an offence under this section shall be brought in England or Wales except by or with the consent of the Director of Public Prosecutions.

(5) Nothing in subsection (1) of this section shall apply to anything done solely for the purpose of entertainment.

2. The following enactments are hereby repealed, that is to say—

(a) the Witchcraft Act, 1735, so far as still in force, and

(b) section four of the Vagrancy Act, 1824, so far as it extends to persons purporting to act as spiritualistic mediums or to exercise any powers of telepathy, clairvoyance or other similar powers, or to persons who, in purporting so to act or to exercise such powers, use fraudulent devices.

3.—(1) This Act may be cited as the Fraudulent Mediums Act, 1950.

(2) This Act shall not extend to Northern Ireland.

Members who spoke in favour of the Motion were Mr Arthur Colegate (Con.), Mr George Deer (Lab.), Mr C. W. Gibson (Lab.), Mr Leslie Hale (Lab.), Mr Douglas Houghton (Lab.), Mr James Hudson (Lab.), Mr B. Janner (Lab.), the Rev. Gordon Lang (Lab.), Mr L. M. Lever (Lab.), Lt-Col M. Lipton (Lab.), Mr R. J. Mellish (Lab.), Mr T. C. Pannell (Lab.), Mr George Rogers (Lab.), Mr William Ross (Lab.), Mr H. N. Smith (Lab.), Lt-Cdr R. H. M. Thompson (Con.), Mr S. P. Viant (Lab.), Mr David Weitzman (Lab.), Col G. E. C. Wigg (Lab.), the Rev. G. S. Woods (Lab.). The Home Secretary, Mr Chuter Ede, stated that the Bill would be left to free discussion and a free vote. He hoped 'that in some form or other we may through this measure be able to release some of our fellow citizens from an indignity to which at present they feel they are subjected'.

The Council of the Society considered the Bill at their meeting on 7 December. They expressed their entire concurrence in the declared objects of the Bill, namely, to protect honest mediums from prosecution under out-dated Statutes which are in their terms offensive to the whole profession, and at the same time firmly to check fraudulent mediumship.

The Society has for its object the examination in an impartial spirit of various real or supposed faculties usually called paranormal, with a view to bringing within the domain of scientific inquiry those aspects of human personality left unexplored by other branches of psychology. To carry out this difficult and important enterprise, the psychical researcher must be granted the same freedom to pursue his investigations unmolested as is enjoyed by other scientific workers. The mediumistic situation is not as simple as the debate in the House would suggest, and presents no clear-cut alternative between genuine spirit communication and

fraud. The psychical researcher must feel himself secure to state without fear or favour just what his opinion is of any of the incidents under his examination. This, however, necessarily involves making statements that are critical of the powers or alleged powers of mediums. Since it has hitherto been assumed that the existence of the Acts now sought to be repealed has been a bar to the bringing by mediums of libel actions against researchers, the Council felt that the repeal of these Acts, without the provision of some safeguard in this respect, might make it difficult for a researcher to publish the results of his investigations and his comments thereon without running the risk of an action in the Courts. As psychical research is a highly specialised subject, and the proper examination of alleged paranormal phenomena requires a lifetime of study, it was suggested that an ordinary Law Court might be an unsatisfactory tribunal for the decision of such questions. The Council therefore consulted Mr K. E. Shelley, K.C., himself a member of the Society, on this matter and he advises as follows:

While it is undoubtedly of the greatest importance that psychical researchers should have full liberty to publish the results of their investigations and to state their conclusions and even their conjectures with the utmost freedom, in my opinion the ordinary law of libel affords them sufficient protection for this purpose. As the law stands, no criticism can make its author liable to damages provided it is fair and honest, that it is made without malice, and that it contains no misstatement of fact. The circumstance that the criticism reflects the personal beliefs (or disbeliefs) and the prejudices of the author does not matter. All the many reports of investigations contained in the various publications of the Society that I have read fulfil these requirements and, quite apart from the Acts now proposed to be repealed, no successful action for libel could have been brought in respect of any of them. As the Society certainly intends to maintain its existing policy in regard to criticism, I see no reason for it to seek any special protection. Of course, nothing can prevent any person who thinks he has a grievance from instituting legal proceedings, but in my opinion this is an advantage rather than the reverse. Mediums will know that they have the same rights as any other person and can protect themselves from unfair and malicious attacks, and therefore will be the more ready to submit themselves to impartial investigation. Any action brought in respect of a proper criticism is bound to fail (it is surprising how our Courts can master technical details of every kind when these are properly explained) and an unjustified action that deservedly fails may ultimately do so much good by informing the public of the pitfalls and difficulties in this branch of knowledge that the trouble and expense of defending the action would be a valuable investment.

In view of the above advice, it is not felt that there are grounds for making representations in regard to the Bill.

REVIEWS

SPUK : IRRGLAUBE ODER WAHRGLAUBE? Vol. I. By F. Moser. Foreword by C. G. Jung. Baden bei Zurich, Gyr-Verlag, 1950. 342 pp. 9 plates.

This book is the first of two volumes on the poltergeist by Dr Fanny Hoppe Moser, a Swiss biologist, who became interested in occult phenomena in 1914 when she assisted at a sitting for table-turning and levitation with the Berlin medium, Mrs Fischer. In her earlier book, *Der Okkultismus* (Munich, 1935), Dr Moser gave a general survey of the whole field, and although she tried to be as objective as possible, it was clear that she was of the opinion that many so-called psychic phenomena were facts in Nature, and could be investigated by the methods of science.

In her present work Dr Moser has set herself the task of inquiring into the evidence for poltergeist activity, and for this purpose she has examined afresh the records of some cases dating from 1663 to 1946. The accounts of most of these cases are in German, and among them is the very curious story of the phenomena observed by Professor C. G. Jung during his week-ends in a haunted farmhouse in Buckinghamshire in 1920. It is a pity that these events were not reported to the Society for Psychical Research at the time of their occurrence, as, from the notes now printed thirty years later, it is clear that Dr Jung made no effectual attempts to inquire into the objectivity of the sounds, whilst his colleague Dr X. went to bed with a loaded gun as his companion.

As an opening to her survey, Dr Moser has chosen the famous Joller case which began in Stans, a small place south of Lucerne, in 1860 and was recorded by the lawyer, Melchior Joller, in a pamphlet, now very rare, which was issued in Zurich in 1863.¹

The phenomena began with raps, as is so often the case. Joller thought little of them, but when one day in 1861 his small son was found in a fainting condition his father was disturbed to hear that the fit was brought on by raps followed by the alarming appearance of a white misshapen form. Gradually the phenomena increased in number and complexity. The raps on the floor were now as loud as if they had been caused by a heavy mallet, and

¹ A second pamphlet, apparently in reply to normal explanations which had been advanced, was issued the same year. The authorship (attributed by Daumer to Joller himself) is still in dispute. Dr Moser is at pains to reject Daumer's attribution; she also discusses the authorship of the preface to Joller's own pamphlet. In my own copy of the work, which came from the library of Baron Carl Du Prel, the preface is assigned to Perty, which seems to me very probable and which may have been written in by Du Prel himself.

furniture jumped after every blow. Grey shapes were now and then observed flitting about, and soon the case became a sensation. In view of the public clamour a kind of committee of three persons was appointed, including the chief of the local police. On investigation little occurred, but when the Joller family was requested to withdraw from the house the phenomena immediately ceased, only to recommence the moment it returned and actually to increase in violence. A massive walnut table was overturned and an object like a three-tailed cloth appeared and disappeared. Vague shadows as of waving hands were observed, and greyish forms were occasionally seen gliding about. Many pieces of furniture were knocked over; an apple danced about on the floor, and apparitions were seen by the children both in the house and garden. Finally, the Joller family moved elsewhere and the house was locked up.

Dr Moser has taken a great deal of trouble to supplement Joller's account with other contemporary records and the recollections of those still living. Although her industry is to be commended, it does not seem that much was gained from the scientific point of view. Indeed, the publication of these cases is mainly of comparative, academic, and historical interest, and like so many other writers Dr Moser is inclined to be overwhelmed by the enormous weight of testimony to poltergeist activity. She now and then seems to take the attitude of the 'ten thousand-people-can't-be-wrong' school without fully appreciating the basis on which others maintain their attitude of scepticism. Moreover, I suspect that Dr Moser has had but little practical experience in the field. Had she had such experience she would know how utterly different are the accounts of untrained observers from those furnished by skilled investigators, and how the most transparent and simple little deceptions may appear as major phenomena which, at first sight, seems quite inexplicable. When Dr Jung maintains, as he does on p. 260, that parapsychology would do well to make use of the psychology of the unconscious, he is preaching to the long-since-converted. Indeed, could the Jungian psychologists be persuaded to listen to and learn from the parapsychologist, they would sometimes refrain from claiming as examples of the paranormal incidents in the consulting-room that they cannot explain.

It is this inability to explain that lies at the root of Dr Moser's general attitude, and it is connected with her failure to realise the meaning and relevance of much of the material with which she is dealing. The very complexity of poltergeist cases make them very difficult to appraise when all we have are written records from sources which are often suspect in themselves. For, apart

altogether from conscious fraud (which may also be present), we have to contend with the misinterpretation of normal events, which are then fitted into what is supposed to be a supernormal framework, and thus add false notes to the overall picture.

Dr Moser's book, therefore, is of value not so much from the point of view of the practical observer as from that of the historian whose work in this field is also to be esteemed as it enables the student to compare what has been recorded in the past with what is being observed in the present. From the case histories here collected, the reader will be able to learn something of the less spectacular poltergeist records from house and stable and note how, in many instances, those favoured failed even to make the most simple kind of inquiry into the origin of the manifestations. Thus we have the case of the Bavarian Professor of Physics, who, for six months, noted the racketings of a poltergeist in the attic of the house in which he lived, and did not even take the trouble to investigate upstairs. Then we have the case of the student of chemistry in whose apartment a poltergeist raged for some time. Yet, when one day she discovered that over one and a half pints of 'milk' had been apportioned and spilt in one of her rooms, she did not even go to the point of having the liquid analysed! Can it be that many of these events are to be explained by assuming that, *at the time*, they did not strike those participating in them in the same light as they did *later* when describing them to collectors of ghost stories? It is for the reader of Dr Moser's book to judge for himself, and in this he will not be wasting his time. We shall look forward with much interest to the second volume where the authoress has promised to provide a commentary and discussion of the fact recorded in the first.

E. J. DINGWALL

THE ILLUSION OF IMMORTALITY. By Corliss Lamont. New York, Philosophical Library, 1950. Second Edition. xvii, 316 pp. 4 plates. \$3.95.

This is a learned and well-argued defence of the thesis that man is mortal and that there is no life beyond the grave. Most of the arguments are along familiar lines but they are well presented, and generally the case on the other side is fairly considered. Having read Dr Lamont's book, I remain unconvinced by his thesis but I do not think this is due to any defect in the presentation of his case. Its weakness lies not in its arguments but in the system of assumptions on which the arguments rest. If the universe is merely a physical system in which human bodies have evolved from unicellular organisms by a blind operation of the laws of

natural selection, it is indeed unlikely that somewhere in that process an immortal soul was added to man's body, and that in some corner of this naturalistic universe there are to be found a spiritual world and a God who on rare occasions interferes with the natural order.

There is, however, another possibility: that God and the spiritual world are primary and important, and that the natural world is a creation by God for the purpose of attaining spiritual ends. From such a point of view it is not unreasonable to suppose that man is now and eternally a part of that spiritual world, or even that the purpose of evolution was to provide a fit instrument for the human spirit. Such a possibility is not considered by Dr Lamont. Our choice between the possibilities as to a naturalistic or a religious view of the universe must be made on the evidence; the question cannot be decided merely by looking at it from one of the alternative points of view.

The decision is, of course, difficult; that is why there is much difference of opinion on the matter. The assessment of evidence depends on one's valuation of revealed religion and on one's opinion of the finality of the scientific point of view, as well as on one's opinion of the kind of evidence obtained through mediums. Dr Lamont seems to abandon his attempt to be fair in considering the latter kind of evidence. There are obvious difficulties in judging how far the results of seances support a belief in a future life, but consideration of this evidence demands more than reference to 'dear departed grandfather's indulging himself in table-rapping, playing weird tunes on cheap musical instruments or telling the secrets of his past to strange women mediums'.

Even less happy is his reference to experimental psychical research. Dr Lamont repeats criticisms made by Professor Jastrow fourteen years ago of the work carried out at Duke University which were of doubtful validity when they were made and are certainly inapplicable to the work that has been carried out there since that time. He also states that Rhine's successes in ESP have not been repeated in other countries, ignoring the contributions of Soal, Tyrrell, and Whately Carington in this country alone.

R. H. THOULESS

THE MESSAGE OF FATIMA. By C. C. Martindale, S.J. London, Burns Oates & Washbourne, 1950. vii, 183 pp. 10s. 6d.

Fr Martindale's study of the events of 1917, which transformed Fatima from an obscure Portuguese village to a centre of pilgrimage, is a quiet, scholarly, scrupulously accurate piece of research which

should be of considerable value to those interested in the psychical phenomena involved.

He distinguishes carefully between the content of the message given to the three children, calling the peoples of the world to prayer and repentance, and the forms through which they received it. He points out that the human mind is bound to clothe its intellectual perceptions in whatever imagery is most familiar to it—and that that of a Portuguese peasant accustomed to baroque churches is sure to differ from that of (say) an American, or an Indian, or a Greek. (The same truth may be exemplified in a more familiar instance by considering how differently an Englishman and a Chinese will 'see' and paint an identical objective landscape—witness Chiang Yee's delightful pictures of various Oxford scenes.)

Fr Martindale has also carefully separated the evidence given in contemporary documents—the repeated cross-examinations of the children, the reports of their parents, the newspaper accounts of how enormous crowds saw the sun 'spinning' in the sky, and emitting a series of coloured lights, ending with a 'very ugly yellow'—from the later, adult writings and reflections of Lucia, the nun who was the only one of the three who lived to grow up.

None of the original oddities is smoothed away—the suddenness of first vision, with the flash of lightning directing the children's attention to a young girl standing on top of a small tree; the fact that the little Jacinta wanted to offer her some bread and cheese; the sound heard at one later vision by a woman, and at another, later still, by a man, neither of whom saw or heard anything else that seemed unusual, like 'the buzzing of a bee' or 'a horsefly in a bottle'; and the fact that on the 13th October, when most of a crowd assessed at 70,000 strong were awed and terrified by the unexpected apparent movements of the sun, and 'unbelievers' such as the editor of a freethinking newspaper, and a stolid, irritated English domestic brought by her employers saw all the phenomena, several devout persons in the assemblage perceived nothing at all. This, by the way, seems to be an argument against Fr Martindale's tentative hypothesis that the appearances might be explained by 'natural causes'; just as the fact that they were observed from two places some distance away seems to argue against the theory of a collective hallucination brought about by suggestion from one person to another.

This seems likely to remain the standard English work on the subject. Its original sources are quoted (in translation) at considerable length. No attempt is made to attach any disproportionate importance to the form of the visions or the locutions, or to the solar 'miracle'; and the Preface provides a close and interesting analysis of the process through which the mind receives,

symbolises or projects, rationalises, 'tidies' and may even occasionally misinterpret in detail a spiritual experience. It is fascinating to see how closely this description corresponds with what is thought to occur with psychical experience.

Readers should realize, of course, that the book is not written as a piece of psychical research, but in a spirit of deep devotion which holds the service of exact and detailed truth to be an integral part of religion.

RENÉE HAYNES

JOURNAL OF PARAPSYCHOLOGY. Durham, N.C., Duke University Press. \$1.25.

Vol. 14, No. 2, June 1950

In an Editorial on 'Parapsychology and Biology', Professor Rhine discusses the problems raised for parapsychology by such performances as migration and homing. He also considers the bearing of parapsychological findings on basic biological theory.

J. G. Pratt and Esther B. Foster contribute 'A further study of E S P displacement in relation to hits and misses'. They find further evidence that a subject may show a characteristic reaction to the fact of success in an E S P experiment although he is not told of his success. The reaction described is a tendency to produce a pair of misses of the target card, particularly after a direct hit.

There is an account by R. Rose of a preliminary PK experiment in which a positive deviation was obtained which was not significant. The experimenter intends to carry on further with an improved experimental design.

A criticism by C. C. Stevens of Rhine's book *The Reach of the Mind* is followed by an interesting correspondence between Professor Rhine and Mr Stevens in which each tried to clarify his point of view.

M. Skibinsky has tried out the suggestion that E S P subjects will score more highly on targets that are personally significant to them. His results do not support this expectation. The score of his subjects on family names was significantly lower than their score on the symbols in the ordinary E S P pack.

Vol. 14, No. 3, September 1950.

The Editorial deals with the shifting scene in parapsychology. Professor Rhine here discusses the present field of interest in experimental parapsychology and also the further problems for which fruitful methods of experimental attack have not yet been devised.

An article by Dr Soal and Mr Bateman gives an account of experiments done with Mrs Stewart as subject when agents were working in opposition and in conjunction. In both cases it appeared that Mrs Stewart took her responses from one agent, and was not hindered by another agent trying to communicate a different card, or helped by other agents trying to communicate the same card.

Dr Humphrey has carried a stage further her work on separating successful and unsuccessful subjects by the use of tests of personality. By adding to the results of an expansion-compression test and the mid-range and extreme scorers on Stuart's Interest Inventory a further measure derived from items of the Interest Inventory, she finds that she can make a better separation of high and low ESP scorers than the two previous measures gave in combination.

Dr Focht gives an account of the opinion of Ehrenfels on prophecy. It is that future events are known to the over-souls of human groups and that the knowledge of these over-souls is sometimes tapped by individuals. This is obviously one of many possibilities; it is not easy to see, however, in what way this hypothesis could be subjected to experimental test.

The number ends with an acute review reprinted from *Mind* by Professor Price of Rhine's book, *The Reach of the Mind*.

R. H. T.

JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH.

Vol. 44, No. 4, October 1950. New York, A.S.P.R., \$1.50.

George H. Hyslop gives an interesting account of the philosophy of James H. Hyslop and of his contribution to psychical research.

A history of the S.P.R. by W. H. Salter is based on an address he gave to the American S.P.R. in April 1950.

There is an important and fascinating article by Dr C. M. Cooper, M.D., entitled 'An Inherited Baffling Perception and its Uncovering'. This is an admirable little study of the process of unconscious cerebration and the problem of 'hunches' which must be read by everybody interested in paranormal cognition.

D. P.

CORRESPONDENCE

'RETROACTIVE PK' AND THE
CLASSIFICATION OF PHENOMENA

SIR,—Apart from its direct aim, Mr Mundle's paper in *Proceedings* (XLIX, pp. 61-78) is valuable in indicating the difficulties that beset psychical researchers in using current systems of classification, in which the concepts of precognition and PK play a prominent part. Not only is the main theme of his paper the problem of deciding in practice whether both precognition and PK are basic irreducible concepts, or whether apparent cases of one can always be reduced to cases of the other, but also he shows in passing (p. 62) that the term 'precognition' at least is unfortunate, implying as it does that the phenomenon is a form of knowing, an implication which seems not to be justified by our present knowledge of the facts.

The general problem is by no means new. Throughout its history scientific psychical research has been faced with the difficulty of classifying phenomena which tend most perversely to overlap, to masquerade as that which they are not, and to be shy of discovery in isolation. Two main schemes of classification have been used, and neither has been entirely satisfactory. The first, that adopted by F. W. H. Myers in *Human Personality*, is a scheme dictated by the phenomena themselves and using mainly psychological concepts. This becomes inadequate as soon as it is realized that while the surface manifestations, e.g. of trance mediumship or psychometry, may have a close family likeness, the sources upon which they draw vary enormously, perhaps from mere hypermnnesia to the activity of discarnate spirits. The second scheme, that more commonly used or at least assumed today, is based partly on experimental experience and partly on our common *a priori* notions of time, causation, cognition, matter, and mind. This scheme encounters difficulties of the type brought out by Mr Mundle, and I would question here whether we are right to accept these *a priori* factors so readily. It is sufficiently established that the world of the psychical is a very queer one—how queer we have as yet no means of telling. The danger of using our everyday *a priori* framework is that it may lead us to ignore certain possibilities which if followed up would give us a better understanding of what we have to deal with. That this particular framework is not necessary to our thinking is shown by its partial abandonment in modern physics.

Theory must not run too far ahead of the facts. But it is idle to pretend that we can ever wholly divorce fact from theory, or

refrain from building hypotheses until we have had a good look at the bare data. We have to view the facts in the light of some theory, but we can at least realize that it is a theory and make it as explicit as possible, hoping by so doing to reveal its weaknesses and its potentialities.

My aim here is to show that by extending the scheme followed by Mr Mundle we find room for a new type of phenomenon not so far considered. If such a phenomenon is to be found, it should not be very difficult to produce *prima facie* evidence for its existence. On the other hand, if such *prima facie* evidence is discovered, it will increase the problems of using this type of scheme at all. Difficulties of distinguishing between phenomena will be multiplied, and it may be necessary to abandon the scheme altogether.

As things are, Mr Mundle recognises two main groups of phenomena, one cognitive and the other conative. The cognitive group consists of mental events related to other events, mental or physical, in the past (retrocognition), the present, and the future (precognition). In the conative group we have an apparently different type of mental events, bearing a relationship of a different kind to physical events (PK) or mental events (telepathic suggestion) in the present or near future. (Mr Mundle does not mention telepathic suggestion, but it fits in naturally here.)

I am being purposely vague in my description of these two groups. An attempt at better definition would only lead to distortion. It will be seen that as they stand the two groups are not on all fours. While the cognitive group covers past, present and, paradoxically, future, the conative group, in accordance with normal experience, covers only present and future. But if we admit the possibility of knowing what has not yet happened, we should also be prepared to consider that of causing what has already happened. Or, to put it another way, if a future event can cause a present mental event, why cannot a future mental event cause a present event?

I suggest, therefore, that experiments should be conducted for *prima facie* evidence for what might be called 'retroactive PK'. I cannot find that this has yet been done. In *The Reach of the Mind*, for instance, Rhine says: 'Displacement in ESP tests has become a well-established phenomenon, even though it is not very common. . . . It could not occur in PK in the same way, because of the difference in techniques' (p. 127). Now this is perfectly true of the classical PK experiments. The target is not changed often enough for a displacement of this type to be noticeable —though it is interesting that Rhine mentions indications of a displacement in the other direction, e.g. the 'lag effect' of Pratt

and Woodruff. But experiments with more frequent changes of target would provide an opportunity for such an effect to appear.

If it did appear, however—if, for instance, we found something similar to the - 1 displacements of telepathy—we would at once come up against a difficulty similar to those discussed by Mr Mundle. Such an effect could be explained as a combination of straight PK plus precognition of the next target, assuming that the choice of target was not predetermined. (If it were, the alternative explanation would be even simpler—direct telepathic or clairvoyant knowledge of the target.) And I do not see that this difficulty can be entirely overcome. In all types of experiment some alternative explanation would be possible, though not necessarily probable. The most we could do would be to devise experiments to which arguments of the type used by Mr Mundle on pp. 75-6 would apply, i.e. where to explain the effect produced as being due to retroactive PK would be far more simple and plausible than any other. Such experiments are not easy to devise; but we might be lucky enough to find, for example, a subject who scored well on retroactive PK but got no results in direct precognitive experiments.

It is not of much value to consider such possible experiments in detail unless and until we have at least *prima facie* evidence that retroactive PK does in fact occur. It is sufficient for my present purpose to point out that, as in the cases considered by Mr Mundle here too it would be very difficult to bring about an experimental separation of the various possible factors involved. And this I suggest, may be due at least in part to the inadequacy of our conceptions of these factors.

It seems to me possible that we are going astray in assuming complete separation between knowing and willing, cognition and conation. There are other reasons for this besides the experimental problems so far considered. First, while we seem in ordinary life to know very well what we mean by knowing and willing, the attempts of philosophers and psychologists to analyse these concepts have been singularly unsuccessful. Hence we cannot say with any confidence what the essential features of an act of knowing or an act of willing are. This is paralleled on the other side by the fact that it is also very difficult to give an account of what goes on in an 'act of precognition' or an 'act of PK'. We are therefore on very shaky ground in trying to classify the latter as cognitive and conative acts respectively.

Secondly, what little we do know about the psychology of psi phenomena seems to give some importance to the imagination. And the imagination may be regarded from different angles as either active or passive, as cognitive or conative, or as a neutral

ground between the two. It is true that in laboratory tests for ESP and PK the imagination plays little part, and is normally by-passed for direct motor-action. But outside the laboratory the importance of the imagination is often brought out. One method of achieving success in telepathy seems to be—in rather unsatisfactory metaphorical terms—to make the mind a blank, and then allow images to drift into it; and, on the other hand, to achieve a desired result by supernormal means (i.e. PK, etc.), it sometimes seems to be sufficient to imagine it happening. In both cases, images play an important part, and there is nothing that can be termed an act of cognition in the one case or an act of willing in the other.

All this tends to show that there may be something unsatisfactory for psychical research in adopting without question our everyday categories of cognition and conation. It might be of advantage, at least for a while, to try to forget these divisions, and to make our thought and our language as non-committal as possible. On the analogy of 'psi-faculty' we might talk of alpha-, beta-phenomena, etc., giving a definition of the phenomenon as far as possible in positivist terms. There remains the danger that we would still regard these terms as nothing but new names for our old friends precognition, etc., but the more this danger is recognized and allowed for, the less formidable it will be. We may by this means be able to emancipate ourselves from our earlier ways of thinking, and allow the facts to dictate to us a new and more suitable scheme of reference.

PAMELA M. CLARK

ERRATA

JOURNAL, NOVEMBER–DECEMBER 1950

IN the review of *Into the Unknown*, a description of the panel of investigators was printed as follows (p. 345): 'Three are members of the Society, Lord Amwell, Dr Bendit, and Mr L. A. G. Strong. The other five would probably disdain for themselves the description of expert in psychical research. . . .' The author of the review points out that he wrote 'disclaim', not 'disdain'.

In the last sentence of the review of *In Search of the Miraculous* (p. 352), 'hypothesis' should read 'hypotheses'.

We apologise for these errors.

INDEX TO VOLUME XXXV

THE index to Volume XXXV of the *Journal* (January 1949 to December 1950) is being prepared and will be circulated with the issue for March-April.